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ORIGINAL PAPER

Assessing the Impact of First-Time Imprisonment on Offenders' Subsequent Criminal Career Development: A Matched Samples Comparison

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Abstract Using data from the Netherlands-based Criminal Career and Life-course Study the effect of first-time imprisonment between age 18–38 on the conviction rates in the 3 years immediately following the year of the imprisonment was examined. Unadjusted comparisons of those imprisoned and those not imprisoned will be biased because imprisonment is not meted out randomly. Selection processes will tend to make the imprisoned group disproportionately crime prone compared to the not imprisoned group. In this study group-based trajectory modeling was combined with risk set matching to balance a variety of measurable indicators of criminal propensity. Findings indicate that first-time imprisonment is associated with an increase in criminal activity in the 3 years following release. The effect of imprisonment is similar across offence types.

Keywords Criminal careers · Imprisonment · Developmental trajectory · Propensity scores · Deterrence

Introduction

At the outset of the new millennium 2.5 million individuals are confined in prisons or jails across North America and Western Europe (Council of Europe 2001). While trends in imprisonment rate are not uniformly upward across all countries (Tonry 2007), in most nations rates are at or near all time highs. Much has been written about the causes and

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consequences of rising prison populations, particularly in the United States (e.g., Tonry 2004; Blumstein and Beck 1999) and on the wider social and economic impacts of the increased use of imprisonment (Garland 2001; Manza and Uggen 2006; Pattillo et al. 2004; Western 2006). Far less attention, however, has been given to the topic of this paper—the effects of imprisonment on the subsequent criminality of those actually imprisoned (but see Villettaz et al. 2006; Gendreau et al. 1999). This is surprising in light of the topics manifest importance for public policy, life course research and social science in general (see for a discussion Nieuwbeerta 2006, 2008).

Imprisonment is intended to prevent crime by the physical isolation of offenders, called incapacitation, and by deterring crime in the general population. Imprisonment may also constitute a turning point in the incarcerated offender's criminal trajectory (Sampson and Laub 1993). The theory of specific deterrence, which pertains to the effect of punishment on the sanctioned individual, predicts that the experience will have a chastening effect that reduces criminality (Nagin 1998). Life course research on the other hand suggests that the experience of imprisonment may reduce the incarcerated offender's legal prospects by foreclosing pathways for conventional development (c.f. Nagin and Waldfogel 1998; Sampson and Laub 1993; Western 2002). This in turn may cause imprisonment to increase rather than decrease the imprisoned offenders' future criminal involvement (Bernburg and Krohn 2003; Hagan and Palloni 1990).

Estimating the effect of imprisonment on the subsequent criminal career development of those actually imprisoned is complicated by many factors. One is that a priori even the sign of the effect of the prison experience on subsequent criminality is indeterminate. As suggested above sound arguments can be made for the experience of imprisonment either increasing or decreasing criminality. Further, the effect may be contingent on the number of prior prison experiences, prior criminal career development, and age. Another important complication is that imprisonment is not randomly imposed. The likelihood of incarceration depends upon the seriousness of the crime committed and the offender's prior record and age. If more crime prone offenders are sentenced to prison, recidivism rates among the incarcerated will tend to be higher than among the not incarcerated, independent of any effect of incarceration on criminal career development.

We address these complications by employing a number of analytic strategies. First, we examine whether the association between imprisonment and subsequent criminal career development is contingent on criminal history. Based on typological accounts such as those of Moffitt (1993, 1994) and Patterson and colleagues (Patterson et al. 1989, 1998), imprisonment may exert more of an influence on individuals with criminal histories that are of short duration and involve relatively few offenses than for individuals with a prior criminal trajectory that starts early and shows many convictions. Such contingencies, or the lack thereof, bear on broader theoretical and empirical debates concerning the generality of processes underlying stability and change in crime over the life course and the potential effects of life course events on criminal career development (Sampson and Laub 1993; Moffitt 1993, 2006; Blokland and Nieuwbeerta 2005; Nagin et al. 2003).

Second, because crime is so age dependent, our analysis is designed to strictly control for age. We do this by comparing post-release conviction rates of imprisoned individuals and matched controls who were not imprisoned over identical ages. This analytic strategy also allows us to examine whether the effect of the prison experience is age dependent.

Third, beyond tight controls for age, our primary statistical strategy for inferring the effects of imprisonment on the imprisoned involves an elaboration of an approach laid out in Haviland et al. (2007, 2008) and Haviland and Nagin (2005) that combines group-based

trajectory modeling and propensity score matching. The elaboration involves the use of a generalized from of propensity score matching called risk set matching (Li et al. 2001). With this method those imprisoned at age *t* are matched to those not imprisoned based on pre-imprisonment trajectories of offending as well as a time varying imprisonment propensity score. This combined matching method allows us to show that the imprisoned and their non-imprisoned controls are comparable on a wide range of factors including prior record and seriousness of conviction offense that might otherwise bias our estimates of the effect of imprisonment on criminal career development.

Our analysis is based on the large "Criminal Career and Life-course Study" data set that includes the conviction histories thru 2002 of a sample of individuals convicted in the Dutch courts in 1977. We find that for all trajectory groups first time incarceration is associated with a positive and statistically significant increase in offending.

The Effects of Imprisonment

The intellectual roots of modern deterrence research lie in the classical school of legal philosophy (Beccaria 1995), which theorized that punishment discourages future lawbreaking by punished individuals by increasing their perceptions of the severity and certainty of punishment. If the experience of punishment is sufficiently distasteful some of the punished may indeed conclude that it is an experience not to be repeated. However, by the same logic some individuals might also conclude that the experience was not as adverse as anticipated and thus cause them to revise downward their expected utility loss from a future experience with punishment.

The structure of the law itself may also cause previously convicted individuals to revise upward their estimates of the likelihood and/or severity of punishment for future lawbreaking. The criminal law commonly prescribes more severe penalties for recidivists. For example, sentencing guidelines routinely dictate more severe sentences for individuals with prior convictions. Prosecutors may also be more likely to prosecute individuals with criminal histories.

Notwithstanding the statutorily prescribed increase in penalties for repeat offenders, the experience of punishment could also trigger a downward revision of expectations about the certainty of punishment. Research on substance abuse and driving under the influence finds a positive effect of experience with punishment on subsequent offending (Paternoster and Piquero 1995; Piquero and Paternoster 1998). Pogarsky and Piquero (2003) propose the idea of a "resetting" effect to explain this positive association. The resetting effect is an application of the concept of the "gambler's fallacy" (Clotfelter and Cook 1993; Gilovich 1983). Just as folk meteorology holds that "lightning never strikes twice in the same place," the gambler's fallacy holds that bad/good things do not run in quick succession. Consequently the experience of punishment may lead to a decrease not an increase in the punished individuals' estimate of the certainty of being punished which, in turn, may encourage them to offend more frequently.

Beyond altering expectations, the experience of punishment may affect the likelihood of future crime by increasing or decreasing the attractiveness of crime itself or by expanding or contracting alternatives to crime. While imprisoned the individual may benefit from educational or vocational training that increase post-release non-criminal income earning opportunities (MacKenzie 2002). Other types of rehabilitation are designed to increase the capacity for self restraint when confronted with situations, like a confrontation, that might provoke a criminal act such as violence (Cullen 2002).

There also are, however, many reasons for theorizing that the experience of punishment might increase an individual's future proclivity for crime. Prisons might be 'schools for crime' where inmates learn new crime skills even as their non-crime human capital depreciates. Associating with other more experienced inmates could lead new inmates to adopt the older inmate's deviant value systems or enable them to learn 'the tricks of the trade' (Adams 1996; Hawkins 1976; Steffensmeier and Ulmer 2005). Being punished could also elevate the offender's feelings of resentment against society (Sherman 1993) or strengthen the offender's deviant identity (Matsueda 1992).

The experience of punishment may also increase future criminality by stigmatizing the individual socially and economically. There is much evidence showing that an important part of the deterrent effect of legal sanctions stems from the expected societal reactions set off by the imposition of legal sanctions (Williams and Hawkins 1986; Nagin and Pogarsky 2003; Nagin and Paternoster 1994). Prior research has found that individuals who have higher stakes in conformity are more reluctant to offend when they risk being publicly exposed (Klepper and Nagin 1989). While the fear of arrest and stigmatization may deter potential offenders from breaking the law, those that have suffered legal sanctions may find that conventional developmental routes are blocked. In their work on the 500 Boston-delinquents initially studied by Glueck and Glueck (1950), Sampson and Laub (1997) have called attention to the role of legal sanctions in what they call the process of cumulative disadvantage. Official labeling through legal sanctions may cause the offender to become marginalized from conventionally structured opportunities and conventional others, which in turn increases the likelihood of their subsequent offending (Bernburg and Krohn 2003). Sampson and Laub (1997) propose that legal sanctions may amplify a 'snowball' effect that increasingly 'mortgages' the offender's future by reducing conventional opportunities. Several empirical studies support the theory that legal sanctions downgrade conventional attainment (Freeman 1996; Nagin and Waldfogel 1995, 1998; Sampson and Laub 1993; Waldfogel 1993; Western 2002; Western et al. 2001) and increase future offending (Bernburg and Krohn 2003; Hagan and Palloni 1990).

Both deterrence- and life course research suggest that the effect of the experience of punishment may depend upon an individual's developmental history particularly as it relates to criminality. Stafford and Warr (1993) for example, suggested that the effect of punishment may depend on ones history of punishment and punishment avoidance and Pogarsky and Piquero (2003) only found evidence of resetting effects with the least experienced offenders. Finally, deterrence researchers have suggested that a small number of habitual offenders may offend without even considering the expected punishment at least at current threat and severity levels. In addition, typological developmental theories like those of Moffitt (1993, 1994) and Patterson and colleagues (Patterson et al. 1989, 1998) suggest that imprisonment is most likely to influence the development of offenders of the adolescence limited type, and have less effect on the behavior of life course persistent offenders. Results showing that school grade retention was a negative turning point for most boys, but not for boys on the trajectory of persistent physical aggression, suggest that imperviousness may be reached early in the course of development (Nagin et al. 2003).

In sum, while stressing different causal mechanisms both deterrence and life course research provide sound theoretical arguments for why the experience of being sanctioned, in particular being sent to prison, might either reduce or exacerbate subsequent offending. Further, both lines of research suggest that the effect may be contingent on developmental history with those with the longest histories of criminality least effected.

Prior Research and Current Focus

Two systematic reviews of the literature on the effect of imprisonment on subsequent criminality have recently appeared in Nagin et al. (2008) and Villettaz et al. (2006). Nagin et al. builds upon and extends the review for the Campbell Collaboration of Villettaz et al. Both use a similar format for dividing the literature: experimental studies, matching studies, and regression studies and reach generally similar conclusions. We summarize here the conclusion of the Nagin et al. review.

Out of 3,000 studies examined, only five involved random assignment of incarceration, either by controlled of natural experimental design. Most concerned nonviolent juveniles or convictions for minor offenses and compared very short custodial sanctions with some form of non-custodial sanction. A majority of treatment effect point estimates in the experimental studies were in the direction of a criminogenic effect of a custodial sanction but effects were generally not statistically significant.

The first study, by Bergman (1976), compared offenders randomly assigned to either probation or imprisonment. Results showed those assigned to probation indeed recidivated less than those sent to prison. A second study by Schneider (1986) reports on a experiment in Bois, Idaho involving random assignment to either a restitution program of traditional correction programs (incarceration and parole). While both prevalence and incidence rates of recidivism appeared lower in the restitution group, differences were not statistically significant. A third study by Barton and Butts (1990) examined the impact of intensive supervision versus incarceration on recidivism in a sample of over 500 youths. Like Schneider, these researchers find that after 2 years the average number of criminal charges of those randomly assigned to supervision does not differ significantly from that of those incarcerated.

Two other studies used data from outside the US. The first involved a natural experiment in the Netherlands (Van der Werff 1979). Thanks to a royal pardon at the occasion of the wedding of first-born princess Beatrix, people having to serve an unsuspended prison sentence up to 14 days who had committed their offence before January 1, 1966 had their sentence automatically suspended, while people receiving sentences for offenses committed after this date had to serve their prison sentence. Except for the date of their offence, both groups of offenders thus could be considered similar. After a 6-year follow-up recidivism rates for traffic and property offenses were similar. Violent offenders though, who had their sentence suspended re-offended significantly less than violent offenders who had to do prison time.

Finally, an experiment in the Swiss canton of Vaud (Killias et al. 2000) involved random assignment to community service and incarceration. Both sentences had a maximum duration of 14 days. While many of these offenders had prior convictions, it remains unclear whether they had any prior prison experience. A first evaluation after a two-year follow-up showed those who were incarcerated to have more police contacts than those sentenced to community service. The criminogenic effect of imprisonment seemed not to be mediated by the detrimental effect of imprisonment on the offenders' professional or personal life. A second follow up after 11 years (Killias et al. unpublished manuscript) however, found no significant differences in police registrations between the two groups.

In their recent systematic review Nagin et al. (2008) also show that out of the 3,000 studies held worldwide studying the relative effects of custodial versus non-custodial sanctions, only 33 were matching studies or regression studies.

A total of eleven matching studies were identified with six involving serious offenders. In all 11 studies a majority of point estimates were in the criminogenic directions although

not consistently statistically significant. The largest number of studies, 31 was regression-based. There was considerable heterogeneity in these studies both in terms of their quality and the types of custodial and non-custodial sanctions that wee examined. Twenty-two studies reported a majority of criminogenic point estimates of the effect of incarceration on recidivism. Seventeen studies reported at least one statistically significant criminogenic point estimate and seven reported a statistically significant preventive point estimate.

Based on this review Nagin et al. (2008) concluded that "compared to non-custodial sanctions, the experience of incarceration has a null or mildly criminogenic impact on future criminal involvement." but go on to emphasize that "..this assessment is not sufficiently firm to guide policy, with the exception that it calls into question wild claims that imprisonment has strong specific deterrent effects."

Several caveats about the Nagin et al. review, however, are in order for our purposes here. The review makes no attempt to assess the crime control benefits that may accrue from incapacitation as investigated, for example, by Sweeten and Apel (2007) and (Wermink et al. (2009). It also does not speak to the contributions of competing mechanisms (e.g., stigma and special deterrence) to the net effect. This is important because an elegant analysis by Helland and Tabarrok (2007) suggests that in isolation of other mechanisms such as stigma there may a specific deterrent of the threat of more severe punishment for a repeat offense.

Our overall evaluation of the literature on the effects of imprisonment coincides with that of Nagin et al. (2008). Many of the non-experimental studies are problematic due to the limited number of variables controlled for particularly as they relate to the offender's prior record and the characteristics of the present crime that triggered the sanction. On the other hand, much of the experimental evidence involves juveniles whose records are generally sealed and/or involve extremely short periods of incarceration of minor offenders. We, thus, believe that more research on this very important topic is needed.

This Study

This paper aims to advance the literature on the effect of imprisonment on the imprisoned in several ways. First, many of the potentially criminogenic effects of incarceration stem from its stigmatizing effects or the individual's realization that incarceration is not as unpleasant as anticipated. Both of these effects are more likely consequences of the first experience of incarceration than of a repeat experience. Thus, we target our analysis on estimating the effects of first-time imprisonment. The focus on first-time imprisonment also allows us to avoid having to account for feedback effects between imprisonment and crime whereby imprisonment affects the likelihood of crime which in turn affects the likelihood of imprisonment. Accounting for such an endogenous relationship greatly complicates the analysis and also increases the risk that our estimate of the effect of imprisonment on recidivism is contaminated by biases due to endogeneity. We recognize that the focus on first-time imprisonment limits the generality of our findings, but balanced against this cost is the benefit of elimination of important sources of bias.

Second, perhaps the most serious threat to the validity of a finding suggesting that imprisonment is criminogenic stems from the observation—made in the opening paragraph of this analysis—that imprisonment is now the sanction reserved for the most serious crimes and for individuals with the most serious histories of crime. Thus, great care must be given to accounting for a selection process that will tend to make the imprisoned group disproportionately crime prone compared to the not imprisoned group. We use statistical

procedures which are specifically designed to balance a variety of important measurable indicators of criminal propensity.

Third, as also discussed, criminological and life course theory suggest that the effect of imprisonment may depend upon the length and frequency of prior offending. Thus, we estimate trajectory group specific effects.

Fourth, we give special attention to controlling for one of the key correlates of criminal propensity—age. Specifically, we compare post-release conviction rates of imprisoned individuals and matched controls over identical ages. We estimate the effect of first-time imprisonment at each age between age 18–38 on post-release conviction rates. This analytic strategy also allows us to examine whether the effects of imprisonment are age dependent.

Data and Measures

Data

The analysis used data from the 'Criminal Career and Life-course Study (CCLS), a large-scale longitudinal study (Nieuwbeerta and Blokland 2003). The CCLS is based on a representative sample of 4% of all cases of criminal offenses that were tried in the Netherlands in 1977 (the CCLS builds on the work of Block and Van der Werff (1991) and Van der Werff (1986).[1] The total sample consists of 5,164 individuals (see also: Blokland et al. 2005).

The criminal careers of the offenders in the sample were reconstructed using abstracts from the general documentation files (GDF) of the Criminal Record Office ('rap sheets'). The GDF contain information on every criminal case registered by the police at the Public Prosecutor's Office. While the GDF contain information on all offenses that have lead to any type of judicial action, here we use only information on those offenses that were either followed by a conviction or a prosecutorial disposition due to policy reasons—concisely referred to below as convictions, thereby excluding cases that resulted in an acquittal or a prosecutorial disposition due to insufficient evidence. All convictions before 1977 as well as any convictions in the period 1977–2002 were recorded. For each individual in the sample we thus obtained data on the number of convictions per year—including the 1977 charge—starting at age 12 (the minimum age of criminal responsibility in the Netherlands) up to the year 2002[2] When applicable the GDF extracts also contain information on the length of imprisonment following a conviction.

To obtain data on possible time varying variables that might confound the effect of imprisonment, data on life circumstances were collected from population registration data (GBA) and added to the conviction histories. Since 1938 all Dutch citizens are registered in their municipalities. Personal records in the population registration contain information on marriage and fertility history and date of death. Prior to electronic registration, that is prior to 1994, personal record cards were used that were sent to the next town of living every time a person moved. For individuals who had died before 1994 these personal record cards were retrieved from the Center for Genealogy and Heraldry. Based on personal

[1] All cases ruled upon by a judge and all cases 'dismissed for policy reasons' or 'dismissed for technical reasons'—for example due to failing evidence—by the Public Prosecutor.

[2] Note that in the Netherlands a person is not given a 'blank sheet' upon becoming an adult. The extracts used thus contain information on both juvenile and adult offenses.

details from 1977, we were able to trace 89.4% of the original sample—over half those we were unable to trace were neither born nor residing in the Netherlands in 1977—leaving a total of 4,615 individuals in the sample to be analyzed.

For various reasons we restricted our attention to a subsample of the 4,615 individuals for whom we have life history data. Because women are likely to follow different developmental trajectories of crime, the effect of imprisonment on their criminal careers deserves a separate analysis. We therefore excluded all women ($N = 424$) from our dataset. Second, our primary analysis focused on the effects of imprisonment in the 18–38 age range on conviction rate in the ensuing 3 years. For this reason we excluded the 17 individuals who died within 3 years subsequent to their year of first imprisonment. Third, we limited the analysis to persons who up to age 18 had not been imprisoned. As a result we excluded another 342 men. Fourth, as expounded below we contrasted those first imprisoned with those convicted but not imprisoned. We thus limited our sample to persons who had at least one conviction between ages 18 and 38, excluding about a third of the remaining men. These sample restrictions resulted in an analysis sample of 2,790 individuals of whom 1,475 were first-imprisoned between age 18 and 38 and 1,315 that were convicted but not imprisoned.

Treatment: First Time Imprisonment

The 'treatment' that we wish to assess is the effect of first-time imprisonment between age 18 and 38. Data on imprisonment were taken from the GDF extracts. All sentences involving detention, including being placed in a reformatory school, irrespective of their length were counted as imprisonment. Of all 4,615 offenders on which data on life circumstances was available, 40.3% was sentenced to imprisonment at least once during their criminal careers up to 2002. Offenders were first-time imprisoned as young as age 12. The mean age of first imprisonment was 22 years; the median age of first imprisonment was 20.

The decision to make estimation of the imprisonment effect between ages 18–38 our primary focus was influenced by several considerations. One was that a sizable number of individuals were first imprisoned over this age (see Fig. 1). About 80% of all imprisoned persons were first imprisoned between 18 and 38. Second, because we were interested in examining whether the effect of imprisonment depends upon prior trajectory of offending, we needed to have a long enough history of offending to create meaningful trajectory groups. Thus, even for the earliest estimate of the effect of imprisonment at age 18 we had offending data from age 12–17. For later ages of first imprisonment we have offending data from age 12 up to 1 year prior to that age to estimate trajectory groups.

The length of first imprisonment ranged from 1 day to 42 months, with an average of 14 weeks. The distribution is, however, very skewed. 31% received a prison sentence of less than 1 month, 49% of 1–6 months and 19% of 6–12 months and only 1% more than a year.[3] For this analysis we set aside the small contingent of individuals with sentences of more than 1 year because of concerns that they be different in unmeasured ways than the rest of our sample and also because our analysis does not attempt to estimate duration effects.

[3] While the penal regime in the Netherlands has become harsher over the years, still more than 80% of the unsuspended custodial sanctions imposed in 1999 (most recent numbers available) were below 12 months. Similar percentages were found in many other European countries such as Belgium, Denmark, Finland, France, Italy, Norway, Sweden and Switzerland (WODC 2003).

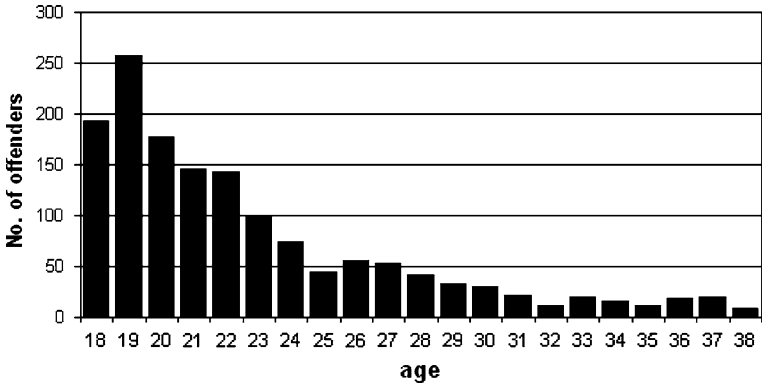


Fig. 1 No. of offenders first imprisoned between age 18 and 38 ($N = 1{,}475$) by age

Outcome Variable: Post Imprisonment Convictions

The outcome variable in our study was the post-treatment conviction rate in the 3 years following the year of imprisonment.[4] Convictions in our study pertain to all felonies mentioned in the Dutch penal code, including the Opium Act and the Fire Arms Act, but excluding all Traffic-act offenses and offenses punishable under other non-criminal acts.[5]

Method: Combined Trajectory Group and Risk Set Matching

Our objective is to estimate the effect of punishment on the punished with non-experimental, observational data. Cochran (1965) suggested that the design of a study aiming to make a causal inference from non-experimental data be organized around the question "How should the study be conducted if it were possible to do it by controlled experimentation?"

[4] To estimate post-treatment yearly conviction rates taking into account the time offenders were 'on the street' and at risk of committing an offense, we calculated the conviction rates only over the period not incarcerated.

[5] While neither official nor self-report data provide a 'true' measure of an individual's criminal behavior (Farrington 1986), we recognize that addressing issues of recidivism based on convictions might introduce bias. If prisons are indeed 'schools of crime' it could be the case that ex-prisoners actually commit more crimes than those not imprisoned, but that at the same time have better learned to go about undetected. The use of conviction data will then underrate the actual recidivism of the ex-prisoners thereby masking actual differences between the imprisoned and not imprisoned group. Thus, to the extent ex-prisoners learn to avoid detection more so than non-imprisoned, convictions may underestimate recidivism for ex-prisoners. On the other hand, the use of conviction data may also result in an overestimate of the treatment effects. Police may be more vigilant towards ex-prisoners and Public Prosecutors may be more inclined to press charges. Yet, given that the police are not always conscious of the adjudication of a particular criminal case, their vigilance is most probably triggered by knowing the offender, rather than his sentence. In addition note that in this study discretionary dismissals by the Public Prosecutor are also counted under convictions, thereby dispelling this possible source of bias at least at the Prosecutors level. It is impossible to judge the overall effects of these potential sources of bias but it is important to keep in mind that all measures of criminality including self reports suffer from comparably important sources of bias.

Two important features of a well designed experiment are (a) good baseline measurements on the subjects of the experiment and (b) comparability of the treated and untreated except for their treatment status. Haviland et al. (2007) demonstrate an approach to causal inference with non-experimental longitudinal data that aims to bring these two features of an experiment to the analysis. The approach is based on propensity score matching within trajectory groups.

Good baseline measurements provide the basis for examining whether response to treatment seems to depend on pre-existing characteristics of the subject population. As already explained, we are particularly interested in assessing whether response to imprisonment depends on the level and time path of prior offending. Group-based trajectory modeling (Nagin 2005) is used to provide baseline categorizations of offending trajectories up to the age of first imprisonment. The method is designed to identify groups of individuals following approximately the same developmental trajectory over a specified period of time for the outcome of interest (e.g., criminal convictions). Fitting a group-based trajectory model to pretreatment, baseline data provides the basis for comparing treated and control subjects who appear similar, in terms of developmental trajectory, prior to treatment. Stated informally, regardless of prison status at a certain age, individuals in the same trajectory group up to that age appear to be headed along the same path, at least so far as criminal offending is concerned. As such, the trajectory groups serve as a baseline measure of response.

The trajectory groups not only provide useful baseline measurements but also contribute to creating balance between the treated and controls on potentially confounding variables. In this regard, it is important to keep in mind that the trajectory groups are based on pretreatment measures of the variable which, after treatment, is the outcome variable. Thus, because of their construction, within trajectory group-based estimation of treatment effects is likely to make a valuable contribution to balancing the pre-treatment measure of the outcome measure (Haviland and Nagin 2005, 2007). Balancing over pre-treatment (i.e., lagged) outcomes is particularly important because it provides protection against biases stemming from selection effects. As will be shown, at each age individuals first sent to prison have significantly more prior convictions than those who are not imprisoned.

However, the use of trajectory groups to achieve balance is empirically based and is not guaranteed to balance covariates. By contrast, methods for achieving balance based on propensity scores (Rosenbaum and Rubin 1983, 1984) and its generalization in the form of risk set matching (Li et al. 2001) are specifically designed to achieve this aim. In a nutshell, the propensity score is the conditional probability of receiving the treatment rather than the control given the observed covariates (Rosenbaum and Rubin 1983).[6] In the current context, the propensity score is the conditional probability of imprisonment at age t given observed covariates up to t. If two individuals have the same propensity score given observed covariates, say a .2 chance of imprisonment, then these observed covariates will be of no further use in predicting which of these two individuals will be imprisoned. As a result, there will be no systematic tendency for the observed covariates to be different between these two individuals.

In this analysis we employ a generalization of propensity score matching called risk set matching (Li et al. 2001). Risk set matching generalizes the propensity score by making it time depend. Specifically, the propensity score is replaced by a hazard model of treatment

[6] A nontechnical survey of methods and results about propensity scores is given by Joffe and Rosenbaum (1999), and for several case-studies, see Rosenbaum and Rubin (1984, 1985), Smith (1997) and Dehejia and Wahba (1999).

at t in which the probability of treatment is a function of pre-treatment covariates measured up to t. Individuals treated at t are matched with individuals who remain untreated as of t but who have approximately the same hazard of treatment.

In many applications, including our own, time of treatment is measured in discrete rather than continuous time (e.g., year of incarceration as opposed to the exact date of incarceration). For such applications a discrete time hazard model is estimated. We model the discrete hazard as a logistic function:

$$p(t|x_{it}) = \frac{1}{1 + e^{\alpha_t + \beta x_{it}}}$$

where x_{it} are possibly time varying characteristic of i measured up to t and α_t is a time specific constant which is estimated by a dummy variable. This specification of the discrete time hazard of first imprisonment is easily estimated as standard logistic regression model on a so-called person-period data file. Its dependent variable then is whether individual i is imprisonment or not at period t and the predictor variables are covariate values measured up to t.

For our analysis model estimation requires the creation of a person-year-data file that contains information on each individual from age 18 up to the year of their first imprisonment or to age 38 whichever comes first. Specifically each individual's record at age t indicates whether the individual was imprisoned or not at t and covariate values measured up to t (e.g., number of convictions for violent offenses up to and including t.) Thus, for individuals who were never imprisoned the data file includes 21 records from age 18–38. For individuals who were imprisoned no further records are created for periods after the year of their imprisonment. Furthermore, the ages after people die or that are after 2002 (i.e., the end of our observation period) are not included. With these restrictions the data-file is composed of 52,630 person-age combinations.[7]

Summarizing, our combined trajectory group and risk set matching method proceeds thru two stages. The first stage involves estimating a group-based trajectory model for the outcome and subjects of interest. For our purposes here, this step involved estimation of a set of 21 trajectory models of convictions that extend from age 12 up to age t ($t = 17, 18, \ldots, 37$). Model estimation includes only individuals who were not imprisoned up to age t. The trajectory models to age t provide our baseline measurements of response to first-time imprisonment at $t + 1$.

In the second stage, individuals who were first imprisoned at each age t were matched with up to three individuals who were convicted at age t but who were not sentenced to incarceration. By matching imprisoned individuals with convicted but not incarcerated controls we aim to measure the effect of imprisonment relative to a non-custodial sanction such as a fine, suspended sentence, a task penalty consisting of a work or training order or some other community sanction or a policy dismissal.[8] This contrast is similar to most existing research which compares imprisonment with different forms of probation. To optimize a variance-bias trade-off we match with up to three individuals instead of only to

[7] Note that since were are interested in the probability of imprisonment conditional on being convicted at time t, propensity score estimates are based on the 5,264 person-years—out of which 1,475 coded as 'imprisoned'—in which people were convicted.

[8] The Dutch suspended sentence is a hybrid form of the Belgian-French *sursis* and the Anglo-Saxon probation. A suspended sentence means the non-implementation of an imposed sentence. A prison sentence up to 1 year may be suspended totally or in part. Prison sentences between 1 and 3 years may be suspended for a maximum of one-third of the total sentence. Prison sentences of over 3 years may not be suspended at all. Other community sanctions include electronic monitoring (Tak 2003).

a single 'control'. To preserve independence across matched sets matching was done without replacement. We also required that the matches on propensity score be within .05. For some imprisoned no three individuals with propensity scores differing less than .05 could be found. Consequently, for these individuals two, one on no 'control' person were matched.

Calculating the Treatment Effect: The Effect of First-Time Imprisonment

After having done the matching, estimating the effects of first-time imprisonment has become straightforward. We examined the effect of first-time imprisonment on conviction rates for the 3 years immediately following the year of the imprisonment at age *t*. For each matched set the effect of imprisonment in period t was measured by the difference in two rates: for the imprisoned individual his conviction rate for the 3 years immediately following *t* and for the matched controls their average conviction rate in the 3 years immediately following *t*. The treatment effect is the average of this difference across the sample of imprisoned individuals.[9]

Pre Imprisonment Characteristics

Pre Conviction Trajectories

Twenty-one separate group-based trajectory models were estimated to provide baseline categorizations of offending trajectories from age 12 to each age from age 17–37. Each such trajectory model was estimated based on the conviction histories of individuals who had not been imprisoned over the period of the trajectory. The zero-inflated Poisson form of the group-based trajectory model was estimated (Nagin 1999, 2005) with the natural logarithm of the Poisson rate parameter for group *j* at age *t*, $\ln(\lambda_{jt})$, specified to follow a quadratic function of age.

[9] Specifically, the outcome estimates and their standard errors were calculated as follows: Let:

$$T_t = \frac{1}{N_t}\sum_{i}^{N_t}\left[y_{it}^{im} - \left(\frac{1}{n_i}\sum_{j}^{n_i} y_{ijt}^{c}\right)\right]$$

where, *i*, an index of the ith imprisoned individual from a total set of N_t individuals imprisoned in t; n_i, the number of controls matched to the ith imprisoned individual; *j*, an index of the jth of the n_i controls matched to *i*; y_{it}^{im}, i's conviction rate in the 3 year period immediately following *t*; y_{ijt}^{c}, the conviction rate of the jth control matched to *i* in the 3 years period following i's imprisonment in *t;* T_t, estimated effect of imprisonment at age *t*.

If n_i were constant across *i*, T_t could be estimated as the difference in the average of y_{it}^{im} and the average of y_{ijt}^{c}. However, if n_i is variable this "difference of the grand means" calculation is not correct. The correct calculation is the average of the individual differences between the imprisoned individual's response and the average response of that treated individual's matched controls.

The variability of n_i also must be taken into account in computing the standard error of T_t. Assuming that y_{it}^{im} andy_{ijt}^{c}, respectively have constant variances σ_m and σ_c, the standard error of the estimate T_t is

$$\frac{1}{N_t}\left[\sum_{i}^{N_t}\left(\sigma_m^2 + \frac{\sigma_c^2}{n_i}\right)\right]^{1/2}$$

where σ_{im} and σ_c are be estimated by the sample standard deviations of y_{it}^{im} and y_{ijt}^{c}.

Note that an increase in the number of controls matched to each ith imprisoned individual (n_i) disproportionally reduces the size of the standard error of the estimate of the treatment effect. This is the reason why we match up to three controls instead matching up to a single control.

Up to age 23 model specifications with more than three groups were not estimable. For models with data beyond age 23 more groups could be identified. As discussed in Nagin (2004) the emergence of more groups is expected in models over more extended age ranges—more data allows for more refined inferences about the time paths of offending trajectories. Notwithstanding, we chose to use a three group representation for all 21 models because 69% of the first imprisonments over the age range 18–38 occurred from age 18–23 and staying with a three group model for all age ranges makes cross-trajectory group comparisons of imprisonment effects far easier to communicate.

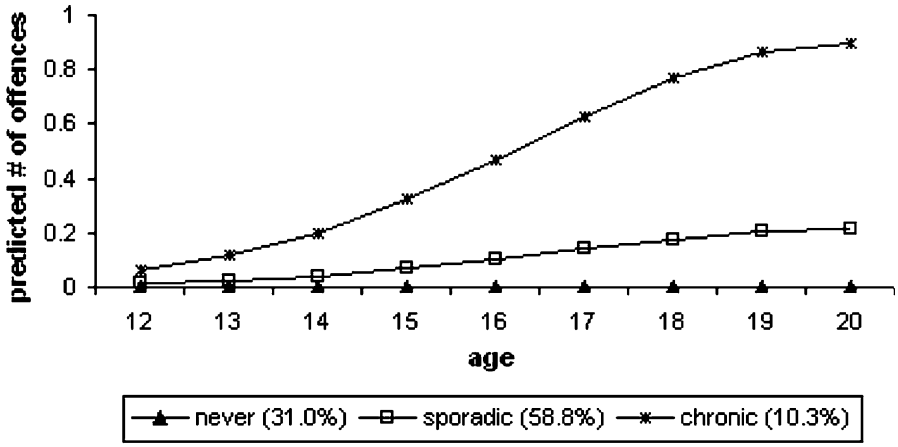
All 21 models were qualitatively similar. The trajectories obtained from the analyzes over the age ranges 12–20, 12–25, 12–30 shown in Fig. 2 are illustrative of the entire set of models. In each model one trajectory group was composed of individuals who had never been convicted up to the model's upper age bound. The size of this group steadily declines from 31.0 to 10.0% as the age range lengthens. Each model also includes a chronic group whose size ranges from 10.3 to 17.2%. For the model over the age range from 12 to 20 the chronic trajectory is rising throughout whereas for the other two models with more extended age ranges the chronic curve begins turning down. The third group in each model is composed of low rate, sporadic offenders. In all models this is the largest group which ranges in size from 58.8 to 72.8%.

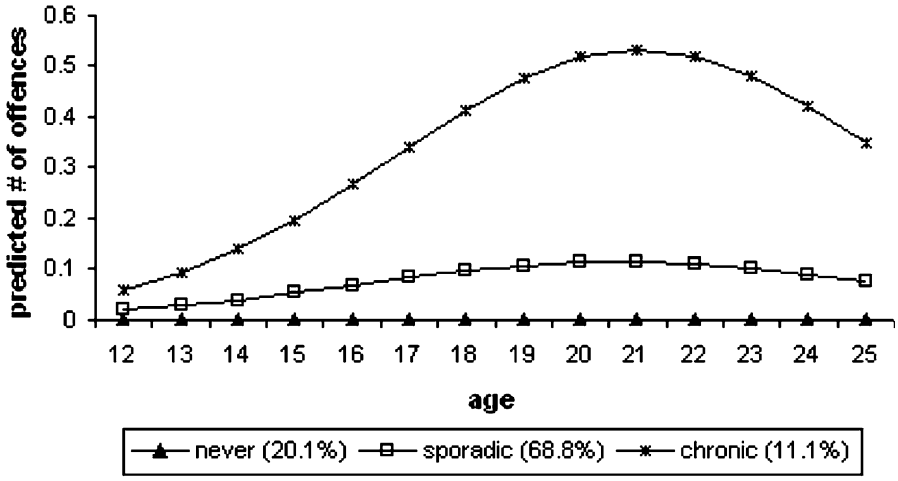
Measuring the Propensity of Imprisonment

While propensity score matching is not a panacea for the problem of nonrandom assignment to treatment status, it is a very useful device for insuring that at least upon measured dimensions the treated and controls are comparable. As others have pointed out (Heckman et al. 1998; Haviland and Nagin 2005), one key element in the success of matching strategies in averting bias is data quality and completeness. In particular, it is important that the specification of the propensity score model includes a rich set of predictors of treatment assignment and of potential confounders of the treatment effect estimate.

Two strands of research provide guidance on which factors are of particular importance in the selection processes into imprisonment. First, prior work on criminal careers and sentencing both in the US (e.g., Johnson 2003, 2006) and the Netherlands (Van Grinsven and Bruinsma 1990; Nieuwbeerta and Leistra 2007) sheds light on factors that may influence judges' decisions. This literature shows that sentence outcomes result not only from the formal considerations on the case prescribed in the law and sentencing guidelines, but also from factors associated with judges' interpretations of individual offender traits. According to Steffensmeier and his colleagues, for example, these considerations revolve around three primary 'focal concerns' of sentencing: offender culpability, community protection (i.e., offender dangerousness), and practical constraints concerning individual offenders (Steffensmeier and Demuth 2000; Steffensmeier et al. 1998).

The second literature of relevance is that on clinical and actuarial risk assessment. Clinical risk assessment refers to the unstructured judgments of trained mental health practitioners based on their theoretical orientation and clinical experience. Actuarial assessments are based on structured assessment instruments for collecting and analyzing data and ultimately making a prediction (Monahan 2006). Research has overwhelmingly demonstrated the superiority of actuarial methods (Grove and Meehl 1996; Aegisdottir et al. 2006; Swets et al. 2000). This conclusion suggests that subtle characteristics of the individuals or their circumstances (e.g., demeanor) that may affect a judge's sentence





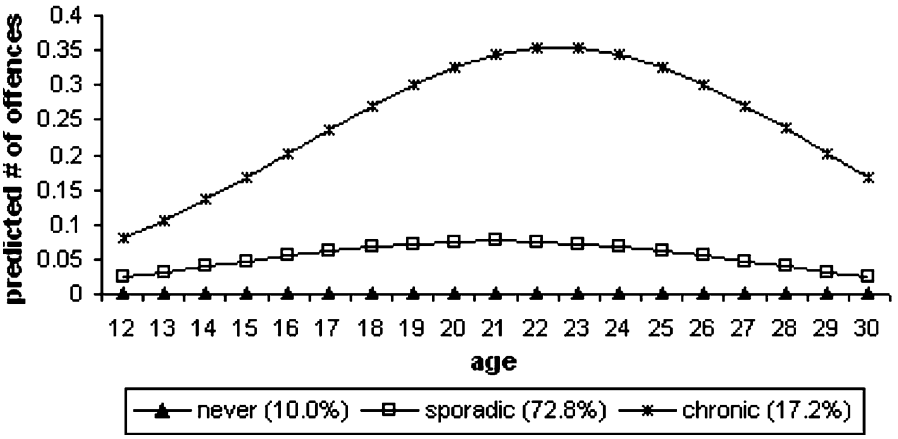


Fig. 2 Trajectories of number of convictions: age 12–20, 12–25, and 12–30

decision but are unlikely to be recorded in the data are also unlikely to bias results because they are not very predictive of behavioral outcomes.[10]

Further perspective on the adequacy of the control variables in our propensity score is provided by the overlap between the variables included in our specification and the information in modern actuarial instruments. Monahan (2006) provides a valuable overview of the types of variables included in such instruments. He delineates four categories. One describes what the individual "is" as measured by age, sex, race, and personality, the second describe what the person "has" as measured by major mental disorder, personality disorder, and substance use disorder, the third describes what the person "has done" as measured by prior crime and violence, and the fourth describes what has been "done to" the person as measured by pathological family environment and victimization. Our propensity model score includes many direct measurements of what the individual "is" and "has done". Measurements of what the individual "has" or "has been done to" are more limited and indirect.

To account for what the person "is", several social demographic factors were entered in as they may—justly or unjustly—be interpreted as signaling future criminal proclivity and thereby contribute to the propensity of imprisonment. We first include a measure for the *age of the offender* in our model.[11] Given the nature of the CCLS-sample personal age and historical period do not coincide for all offenders: offenders have reached certain ages in different historic years. Since over the years the Dutch penal climate has—in relative terms—become harsher, offenders from later *cohorts* might therefore be more likely to be sentenced to imprisonment. To control for cohort effects in the propensity for imprisonment, we also included dummies to divide offenders into three cohorts based on the individuals' age in 1977. The first cohort was comprised of offenders aged between 12 and 21 in 1977, the second of those aged 22–31, and the third of those aged 32 and up.[12] Lastly, we included an indicator of whether the individual is *ethnically non-Dutch*.

To account for perceived offender dangerousness and what the person "has done", we included indicators of offence type and severity in the propensity model. Criminal penalties are aimed at both retribution and prevention. Due to the retributive element, the likelihood (and length) of imprisonment is typically related to the seriousness of the crime as measured for example by whether it involves violence and the individual's blameworthiness. Furthermore, judges may act on the belief that offenders of serious crimes have higher recidivism rates (Fagan and Guggenheim 1996; Gottfredson 1999; Gottfredson and Gottfredson 1988; Kleiman et al. 2007; Vigorita 2003; Von Hirsch 1987). Offence type and severity were measured based on detailed coding of all convictions in the CCLS-data set. For each conviction the underlying section of the criminal code was used to distinguish between types of offenses. These sections also provided information on the maximum

[10] Overall, the justice systems in the US and Netherlands are in many ways similar—both have the same court personnel consisting of judges, prosecutors and defense counsel, both countries provide similar due process rights, and both utilize prison as the most serious sentencing option for offenders. However, a number of key differences also define the two justice systems. Plea bargaining dominates the American system but does not exist in the Netherlands, and juries are a key component of trials in the US but they are not used in the Netherlands. Instead the Dutch system relies on a panel of three judges to determine guilt and sentence. While the American system is more adversarial, relying on cross-examination of witnesses, Dutch judges rely heavily on information in case files.

[11] To allow for non linear relationship between age and imprisonment risk we also included age and age-squared as explanatory variables in the propensity score model.

[12] The cohorts are similar to those distinguished in Blokland et al. (2005) and Blokland and Nieuwbeerta (2005).

penalty applicable for each offense. Based on this detailed information, we constructed dummy variables indicating the *type of offense* that distinguish among 22 types: 6 different types of property offenses, 5 types of violent offenses, 4 types of sexual offenses and 7 other types of offenses. For criminal law offenses we also included an indicator of *offense severity* based on the maximum penalty.[13]

Prior work on sentencing also suggests that the offender's criminal history is relevant for the risk of being sent to prison. Based on their own experience or on criminological research, judges may apply the rule 'the best predictor of future behavior is prior behavior' in their decision to impose a prison sentence. Specifically judges can be expected to more likely impose a prison sentence on individuals with more extensive criminal careers (Mitchell 2005; Spohn 2000). We included a variety of measures of criminal history. First, we included indicators of prior conviction trajectory as measured by an individual's posterior *trajectory group membership probabilities*. Although the trajectory group membership indicators summarized many characteristics of offenders' criminal histories, in an effort to be exhaustive we also include a number of additional measures. These included the number of offenses in the past 5 years (*total number of convictions in the 5 years period prior*) and total prior convictions in the remainder of the criminal history beyond those 5 years (*total number of prior convictions*). We included these *measures for property, violent, other crimes separately*. We also included a variable representing the number of crimes in each conviction, to take into account that a single criminal case can pertain to multiple crimes. Finally, given its significance as a proxy for persistent offending according to some life course theories, we included an indicator whether the *age of first conviction was before age 16.* While our data do not include clinical evaluations of what the offender "has" the extensive data on criminal history and on violence in particular are of course important indirect indicators of conduct disorder and other psychopathologies.[14]

Finally, life course research suggests that life circumstances of the offender could be of relevance —especially whether the *offender is married and/or has (young) children*. Research on marriage and crime in general (Laub and Sampson 2003; Blokland and Nieuwbeerta 2005; King et al. 2007) and studies on the data used in this analysis (Blokland and Nieuwbeerta 2005; Bersani et al. 2009) find that marriage is associated with a lower risk of criminal behavior and recidivism. Furthermore, for offenders with a family imprisonment entails separation from a marriage partner and children. As a result judges may be less likely to sentence married offenders and those with (young) children to imprisonment. We therefore included indicator variables of marital and parental status in the model: two for whether an offender is married and/or has children in a certain year and two indicating the number of years the offender has been married or had children in the past 5 years.

In sum, while we take very seriously the possible caveat that unaccounted for differences between the imprisoned and not imprisoned may be biasing our results, research on focal concerns in sentencing, on clinical versus actuarial prediction of crime and on developmental and life course criminology provides us with some confidence that the

[13] The measure of offense severity ranges from 0 to 20. To improve the interpretation of the coefficients for the effects of type of offense dummies we centered the offense severity around the means of the corresponding offense dummies. For Opium and Gun Law offenses no offense severity measure was available.

[14] The final category in Monahan's taxonomy measures what has been done to the individual. For this category we have no measurements but here we note that our extensive data on prior record is at least in part controlling for the enduring effects of early life experiences.

variables included in our propensity score account for the most important sources of potential bias.

Estimating the Propensity of Imprisonment

Our propensity score is created by a logit regression of imprisoned or not at age *t* on all of the variables in Table 1 measured up to t.[15] The propensity model logit coefficients are reported in Table 1. The results show that the severity and type of the crime committed are especially important. More severe and violent crimes result in a higher probability of imprisonment. Many of the indicators of an offender's criminal history are also of relevance. A more active and chronic history increases the chance of imprisonment. Being married on the other hand reduces the likelihood of imprisonment.

Not all of the coefficients are statistically significant. The statistical (in)significance of the coefficients, however, are not of great concern because the high number of correlated predictor variables in the model might influence the coefficients and their standard errors due to colinearity problems but this does not bias the overall propensity score model.

Matching: Creating Balance

Our method for creating balance on covariates at each age between the imprisoned group and the convicted but not imprisoned comparison group is to sort the entire sample among the three trajectory groups and within trajectory group to match each imprisoned individual with up to three individuals from the comparison group showing like or identical propensity scores. Because most of the potential matches have a low probability of imprisonment compared to those actually imprisoned, it was generally difficult to find three matches for all imprisoned that were close in terms of probability of imprisonment. We therefore first randomly selected approximately 50% of the imprisoned of the 1,475 individual who experienced first-time imprisonment between age 18 and 38 and treat these as the treatment group ($N = 765$). We used the remaining 50 percent of imprisoned as additional potential controls in the ages before they were imprisoned. Because these persons who at later ages are imprisoned often already had high probabilities of imprisonments in the years before they were imprisoned, they provided a very useful pool of potential controls.[16]

Table 2 shows the number imprisoned for the first time at each age and number not imprisoned for the first time at that age for 50% of the full sample. Figure 3 reports the

[15] In estimating the propensity scores only main effects of covariates were estimated—and no interaction terms (see Table 1).

[16] Some readers might question the use of individuals who are imprisoned after t as matched controls for individuals imprisoned at *t*. For such readers it is important to keep in mind several points. First, if we were reporting the results of a randomized experiment in which controls were sentenced to a non-custodial sanction, some of these individuals might subsequently be incarcerated for another offense. If they were excluded from the analysis the bias protection from randomization would be vitiated. Similarly if we were to exclude the later imprisoned as potential matches, this would have in fact been a source of bias. Second, it is important to keep in mind that treatments are administered at specific points in time and that treatment at time t does not in general preclude treatment or not at a later date. Consequently, treatment effects should be understood to be possibly time dependent. For a fuller discussion of these issues see Li et al. 2001).

Table 1 Logistic regression estimates for the propensity score model ($N = 5{,}264$ person-years of which 1,475 coded 'imprisoned')

	Coefficient sign.	SE
Offense characteristics		
Type of offense/conviction		
Simple theft (ref.)	–	–
Aggravated theft	0.792**	0.115
Forgery	0.037	0.272
Embezzlement	−0.088	0.287
Swindling	0.013	0.418
Fencing	−0.031	0.216
Violent theft	2.479**	0.263
Extortion	2.301**	0.444
Threatening	0.973**	0.226
Assault	0.380**	0.127
(Attempted) murder/manslaughter/ culpose death	1.423**	0.329
Flashing	−1.538**	0.339
Rape	2.426**	0.257
Sexual assault	1.309**	0.236
Sexual abuse/other sexual offense	0.668**	0.205
Public order	0.102	0.225
General offending	1.359**	0.400
Offense against authority	−0.546*	0.309
Vandalism	−1.064**	0.241
Other criminal law	−1.241**	0.510
Opium act	0.561**	0.192
Weapons act	0.688**	0.254
Severity of offense (excluding opium and weapons act)	0.215**	0.027
Criminal history		
Trajectory Group membership probabilities		
Never (ref)	–	–
Sporadic	0.564**	0.188
Chronic	0.021	0.435
Number of crimes past 5 years		
Property crimes	0.262**	0.059
Violent crimes	0.036	0.088
Other crimes	0.176**	0.069
Total number of crimes beyond past 5 years		
Property crimes	0.047	0.061
Violent crimes	−0.124	0.104
Other crimes	0.140	0.086
Number of crimes in case of conviction		
Property crimes	0.916**	0.098
Violent crimes	0.510**	0.204
Other crimes	0.665**	0.117
Early conviction (before age 16)	0.011	0.099

Table 1 continued

	Coefficient sign.	SE
Life circumstances		
Marriage		
Number of years married in past 5 years	−0.014	0.046
Married at age 25 (no = 0; yes = 1)	−0.318*	0.147
Children		
Number of years children in past 5 years	−0.013	0.050
Children at age 25 (no = 0; yes = 1)	0.449**	0.180
Social dempographics		
Non-Dutch	0.626**	0.101
Cohort 1	−1.663**	0.115
Cohort 2	−0.918**	0.098
Cohort 3 (ref.)	–	–
Constant	−1.995**	0.256

Effects of age dummies not shown (ref. = age 25)

* statistically significant at $p < .10$; ** statistically significant at $p < .05$

Table 2 Number of imprisoned and non-imprisoned offenders

At age	50% of full sample		Matched sample		% Imprisoned matched
	Imprisoned	Not imprisoned	Imprisoned	Not imprisoned	
18	113	396	92	189	81
19	129	340	98	184	76
20	89	285	63	115	71
21	79	247	66	111	84
22	63	207	45	79	71
23	51	205	37	67	73
24	40	170	28	54	70
25	25	182	18	42	72
26	30	128	23	46	77
27	28	114	19	32	68
28	20	122	14	32	70
29	12	120	9	16	75
30	14	112	10	23	71
31	12	101	9	26	75
32	5	70	4	10	80
33	11	79	8	15	73
34	12	66	11	25	92
35	8	58	5	10	63
36	7	51	4	7	57
37	13	58	8	20	62
38	4	68	4	8	100
Total	765	3,179	575	1,111	75

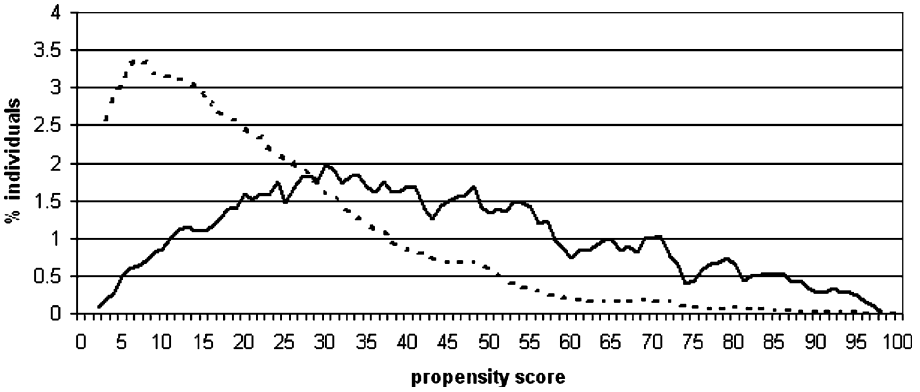


Fig. 3 Distribution of propensity scores for imprisoned ($N = 765$) and convicted/not-imprisoned ($N = 3{,}179$) (*dashed line*). Lines depict the % of the total number of individuals within each group for each value of the propensity score (smoothed using a 5-point moving average)

distributions of the propensity scores for the imprisoned and the not imprisoned. The figure shows that there is sufficient common support: at nearly all propensity scores there are potential controls for the imprisoned. We therefore were able to find matches for 75% of the imprisoned individuals that we attempted to match ($N = 575$).

The .05 caliper requirement for comparability of propensity for imprisonment resulted in our dropping several imprisoned individuals from further analysis because we could not identify even one comparable match for them ($N = 190$). These unmatchable individuals were disproportionately from the high-chronic group and the average seriousness of their convictions was nearly 2.5 times higher than that of the matched individuals. Unmatchable individuals were furthermore 14 times more likely to have been convicted for violent theft and 9 times more likely to have been convicted of rape, whereas the successfully matched individuals were more often convicted for public order offenses and simple theft. For unmatchable individuals the criminal case that led to imprisonment was also more likely to contain multiple accounts of violence and property offenses compared to that of matched individuals. Stated differently in the Netherlands as in the USA, nearly all serious chronic offenders are sentenced to prison.

Our inability to find a match for serious offenders demonstrates what we believe is an important strength of our inference strategy. Propensity score matching only works in situations where there is some discretionary freedom in the process of deciding who becomes treated and who does not. If selection into the treatment is extreme—e.g., when all individuals with certain characteristics are treated or not—the ability to match is lost. Our results show that judges in the Netherlands use their discretionary freedom for less serious offenders, but not for serious chronic offenders. This implies that the results of our analyzes should not be generalized to the entire population of the imprisoned. They apply only to those with low or moderate probability of imprisonment. We regard this as a strength rather than a weakness of our matching-based inference strategy because it clarifies the boundaries of the population for whom the analyzes pertain. By comparison, alternative inference strategies based on regression techniques in general mask the feasible scope of the analysis by extrapolating across individuals that cannot credibly be treated as comparable.

Table 3 Differences between imprisoned and the convicted but not imprisoned—full and matched sample

	Panel A: 50% of full sample			*Panel B*: matched sample		
	Difference between the imprisoned ($N = 765$) and the convicted but not imprisoned ($N = 3{,}179$)			Difference between the imprisoned ($N = 575$) and the convicted but not imprisoned ($N = 1{,}111$)		
	Absolute difference	Two-sample *t*-statistic	Standardized differences in %	Absolute difference	Two-sample *t*-statistic	Standardized differences in %
Offense characteristics						
Type of offense						
Simple theft	−0.058**	−4.488	−17	−0.011	−0.547	−3
Aggravated theft	0.040**	5.056	24	−0.016	−0.572	−3
Forgery	0.018**	2.594	11	−0.001	−0.141	−1
Embezzlement	0.004	0.569	2	−0.003	−0.361	−2
Swindling	0.009	1.369	6	0.000	−0.058	0
Fencing	0.009*	1.851	8	0.002	0.206	1
Violent theft	0.002	0.124	0	−0.001	−0.124	−1
Extortion	0.047**	5.712	28	0.003	0.818	5
Threatening	0.013**	2.874	14	−0.002	−0.176	−1
Assault	−0.012**	−2.102	−8	0.026	1.155	7
(Attempted) murder/ manslaughter/culpose death	0.127**	6.975	30	0.001	0.161	1
Flashing	−0.008**	−1.912	−7	0.005	0.876	5
Rape	−0.004	−1.158	−4	−0.007	−0.825	−5
Sexual assault	−0.020**	−3.153	−11	0.004	0.341	2
Sexual abuse/other sexual offense	−0.011*	−1.644	0	−0.005	−0.436	−3
Public order	0.005	1.395	6	0.006	0.605	4
General offending	−0.021**	−4.642	−15	−0.003	−0.488	−3
Offense against authority	−0.049**	−9.434	−28	0.003	0.528	3
Vandalism	−0.069**	−10.607	−33	0.002	0.220	1
Other criminal law	−0.012**	−4.389	−13	−0.001	−0.380	−2
Opium act	−0.003	−0.345	−1	0.001	0.093	1
Weapons act	−0.006	−1.167	−4	−0.004	−0.474	−3
Severity of offense (excluding opium and weapons act)	0.388**	6.690	27	0.024	0.290	2
Criminal history						
Trajectory group membership probabilities						
Never	−0.024**	−2.298	−9	−0.005	−0.313	−2
Sporadic	0.008	0.710	3	−0.003	−0.172	−1
Chronic	0.016	1.317	5	0.008	0.502	3
Number of crimes past 5 years						
Property crimes	−0.155**	−4.811	−17	0.054	0.704	4
Violent crimes	−0.086**	−4.953	−18	0.048*	1.647	10
Other crimes	−0.083**	−4.080	−15	0.029	0.683	4

Table 3 continued

	Panel A: 50% of full sample			*Panel B*: matched sample		
	Difference between the imprisoned ($N = 765$) and the convicted but not imprisoned ($N = 3{,}179$)			Difference between the imprisoned ($N = 575$) and the convicted but not imprisoned ($N = 1{,}111$)		
	Absolute difference	Two-sample *t*-statistic	Standardized differences in %	Absolute difference	Two-sample *t*-statistic	Standardized differences in %
Total number of crimes beyond past 5 years						
Property crimes	0.373**	6.978	30	−0.011	−0.253	−1
Violent crimes	0.059**	2.671	11	0.007	0.320	2
Other crimes	−0.004	−0.136	−1	0.012	0.436	3
Number of crimes in case of conviction						
Property crimes	0.020**	2.900	13	−0.011	−1.231	−7
Violent crimes	0.167**	7.983	38	0.004	0.202	1
Other crimes	0.025**	2.085	9	−0.018	−0.984	−6
Early conviction (before age 16)	0.045**	2.548	10	−0.027	−1.052	−6
Life circumstances						
Marriage						
Number of years married in past 5 years	−0.342**	−5.395	−20	0.019	0.214	1
Married at age 25 (no = 0; yes = 1)	−0.085**	−5.314	−20	0.007	0.293	2
Children						
Number of years children in past 5 years	−0.263**	−4.112	−16	0.024	0.272	2
Children at age 25 (no = 0; yes = 1)	−0.056**	−3.375	−13	0.004	0.184	1
Social demographics						
Non-Dutch	0.103**	6.640	29	0.017	0.788	5
Cohort 1	−0.074**	−3.895	−15	−0.047*	−1.681	−10
Cohort 2	0.028	1.424	6	0.053*	1.826	11
Cohort 3	0.046**	2.731	11	−0.006	−0.258	−2
Propensity score	0.216**	25.685	112	0.004	0.399	2

Since in matched sample the no. of mathed controls differs, the controls are weighted by "1/no. of convicted". The weighted no. is 575

* Statistically significant at $p < .10$; ** statistically significant at $p < .05$

Results: Balance

Table 3 reports the extent to which our matching procedure was successful in creating balance on many important characteristics of the imprisoned and their matched controls. The difference between the imprisoned and the comparison group is calibrated by two types of statistical comparisons. One is the conventional two-population t-test. The other is a standardized difference statistic—in percentages—suggested by Rosenbaum and Rubin

(1985:36).[17] When the absolute value of the standardized difference D is greater than 20, Rosenbaum and Rubin recommend concluding that the characteristic in question is out of balance between the two groups.

Panel A of Table 3 reports comparisons of the 50% of the full sample of individuals who were imprisoned for the first time over the 18–38 age range ($N = 765$) with individuals who were convicted but not imprisoned. Clearly, prior to matching the imprisoned differ in important ways from the convicted but not imprisoned: for all variables the t-test show that that the imprisoned differ statistically different from the convicted but not imprisoned. Moreover, for many variables the standardized difference D is greater than 20. The imprisoned were more likely to be convicted for crimes of violence, they had more extended prior criminal histories, were less likely to be married or have children, and were more likely to of non-Dutch ancestry.

Panel B of Table 3 shows that our matching procedure was very successful in achieving balance on the characteristics generally assumed relevant for both sentencing and criminal behavior. Before matching there were many imbalances, but after matching, the number of significant differences is down to none according to the Standardized Difference (D) or only a single variable (i.e., cohort) according to t-statistics. Importantly, the combination of stratification by trajectory group and matching within trajectory had excellent success in balancing conviction rates up to each age (not shown). This implies that we can be confident that differences between the post imprisonment conviction rates of the incarcerated and the comparison group are unlikely to be reflection of differences in prior conviction rates or other characteristics between the 'treatment' and 'control' group.

Results: Treatment Effects of First-Imprisonment

Table 4 reports estimates of the effect first-time imprisonment overall and by crime type and trajectory group. The overall estimates of the effect of first-time imprisonment which aggregate across trajectory group and age is positive and highly significant for all crime types. The positive sign of the treatment effect estimates implies that, on average, the experience of first-time imprisonment exacerbates criminality. Not only are the estimated effects highly statistically significant, they are also substantial in magnitude. The overall

Table 4 Average treatment effects of imprisonment on the post release annual conviction rates

	Never		Sporadic		Chronic		All	
	T	t-statistic	T	t-statistic	T	t-statistic	T	t-statistic
Property crimes	0.56	2.37	0.91	4.38	1.30	2.33	0.87	5.47
Violent crimes	0.42	3.23	0.17	2.44	−0.09	−0.50	0.20	3.48
Other crimes	0.33	2.67	0.23	3.08	0.44	1.60	0.28	4.30
All crimes	1.32	3.83	1.30	4.98	1.66	2.13	1.35	6.49
No. of imprisoned	364		144		67		575	

[17] The formula for the standardized difference statistic—in percentages—as suggested by Rosenbaum and Rubin (1985:36) is:

$$D = \left(\frac{\bar{X}_w - \bar{X}_n}{\sqrt{\frac{s_w^2 + s_n^2}{2}}} \right) \times 100$$

Table 5 Average treatment effects of imprisonment on the post release annual conviction rates—by age

At age	Property crimes		Violent crimes		Other crimes		All crimes		No. of imprisoned
	T	*t*-statistic	*T*	*t*-statistic	*T*	*t*-statistic	*T*	*t*-statistic	
18	1.676	2.908	0.179	1.149	0.349	1.646	2.203	2.964	92
19	0.611	1.537	0.380	2.187	0.402	2.619	1.394	2.729	98
20	1.401	3.122	0.224	1.322	0.722	3.501	2.347	3.927	63
21	1.177	2.107	0.281	1.247	0.027	0.145	1.485	2.132	66
22	0.719	1.654	0.333	1.648	0.277	1.288	1.329	2.160	45
23	0.727	1.773	0.378	1.933	0.193	1.021	1.298	2.242	37
24	0.615	1.178	0.257	1.441	0.324	2.207	1.197	2.166	28
24–25	0.644	1.894	0.233	1.869	0.333	2.425	1.210	3.184	46
26–27	0.420	1.171	−0.045	−0.463	−0.031	−0.206	0.344	0.821	42
28–30	0.141	0.586	−0.246	−1.691	0.500	1.344	0.394	0.787	33
31–38	0.108	0.912	−0.010	−0.081	−0.174	−1.148	−0.076	−0.299	53
All	0.867	5.469	0.201	3.481	0.279	4.296	1.347	6.489	575

treatment effect across for all crimes is 1.35 convictions. This implies that the conviction rate of the imprisoned in the 3 years following the year of their imprisonment is 1.9 times higher than the average of their matched controls. For property crimes, violent crimes, and other crimes, the treatment effects are 0.87, 0.20, and 0.28 convictions, respectively, which are, respectively, 2.0, 1.8, and 1.8 times higher than the average conviction rates of their matched controls.

Within trajectory group with one exception all treatment effect estimates are positive and ten of twelve are significant at the .05 level for a two-tailed test. For all crimes combined the sizes of the effect are similar across trajectory groups. Statistical tests for differences in the overall rate across trajectory group are not close to significance at conventional levels. Inspection of the point estimates for the specific crime types suggest some possible differences across trajectory group but with the exception of violent crime none are statistically significant.

Finally, Table 5 reports estimates of treatment effects by age. Because of the small sample sizes after age 23 treatment effects are estimated by age groups. For all crimes combined the effect estimates up to age 25 are statistically significant at the .05 level. After that age none of the estimates are statistically significant at conventional levels. While the lack of significance may be attributable to statistical power, there is a fairly clear pattern of the treatment effect size declining with age. Indeed a regression of the absolute effect size with age finds a negative and significant decline. The findings by crime type are similar—generally significant effects at younger age that decline in magnitude to statistical insignificance at older ages.

One needs to realize, however, that at each age the treatments effects are measured as the difference between the number of convictions of the imprisoned and the average number of convictions of their controls. So the treatment effect is measured in absolute terms and not relative to the level of offending at each age. This is important since crime is age-graded and for both the treated and their controls crime declines with age. As a result so does the absolute treatment effect.

Discussion and Conclusions

Our findings suggest that the experience of first-time imprisonment is associated with an increase in criminal activity in the 3 years following release. Thus, the analysis implies that on balance the criminogenic effects of imprisonment on the imprisoned are larger than any preventive effect that might stem from special deterrence.

It is important to recognize that this conclusion is circumscribed by several important considerations. These involve our use of non-experimental data, restriction placed on the sample used for analysis, and the source of the data, the Netherlands. We discuss these in turn.

Because the analysis is based upon non-experimental data the validity of our conclusions rests upon the adequacy of our statistical controls for potentially confounding factors. While the risk of bias from unaccounted factors is an ever present danger in analyzes of observational data, several factors give us a modicum of confidence in our conclusion. Nonetheless, there are substantial reasons for caution that are important to recognize. We first list our reasons for some degree confidence. One is that the propensity score model includes detailed information on the type and severity of the offense, the offenders' history of crime and violence, as well as current life circumstances. These variables measure many of the factors known to be important in sentencing, risk assessment, criminal behavior and recidivism. Second, we were able to demonstrate that after matching we were successful in creating comparability between the imprisoned and non-imprisoned on all measured potential sources of bias. Third, our matching strategy allowed us to identify and set aside individuals for whom there were no comparable matches thereby eliminating an important potential source of bias.[18] Fourth, the relevance and comprehensiveness of potentially confounding variables accounted for in our analysis compares favorably to other studies based on observational data reviewed in Nagin et al. (2008) and Villettaz et al. (2006).

Our reasons for confidence, notwithstanding, reasons for caution remain. We would have liked to have had even richer data on the individual's life circumstances. In this regard, an important gap in our data concerns the individual's alcohol and illicit drug use at the time of his sentencing and relatedly his involvement in alcohol and drug treatment programs. Blokland et al. (2005) show that individuals with serious drug and alcohol abuse problems likely disproportionately comprise a small and very high rate offender trajectory group in the Netherlands. It would also have been desirable to have more precise data on the individual's employment and family involvement at the time of sentencing. All of these life circumstance factors may be sources of unaccounted for bias particularly if they are observable by judges and affect their sentencing decisions. More generally judges may have access to additional information that both affects their sentencing decision and is correlated with recidivism.

The results are based on data from a small Western European country with sentencing practices and a prison system that are quite different and more permissive than the Western country with the highest rate of incarceration by a wide margin—the United States. We think it important to be careful about what this does and does not mean for the generalizability of our findings to the US setting. While sanctions are less severe in the

[18] One reviewer suggested that the practice of setting aside individuals with no suitable matches created rather than prevented bias. We strongly disagree. The individuals for whom we were unable to identify suitable matches all had very high probabilities of imprisonment. This was because they had very long criminal records and/or had been convicted of very serious crimes. These are precisely the types of variables which if left unaccounted for may seriously bias treatment effect estimates. Thus, to have included them in the analysis despite the fact that we had no suitable matches increases rather than decreases the risk of bias.

Netherlands, this does not mean that serious crime is not a major problem in the Netherlands. To the contrary, property crime rates are as high or higher in the Netherlands than the US, and the Netherlands has a serious problem with the equivalent of burglary and robbery in the US. We emphasize this point because the short prison sentences in the Netherlands by US standards does not imply that most of the individuals sentenced to short prison terms are minor offenders by US standards It only connotes that the Netherland's sentence differently than the US.

Still the differences in sanction policy between the US and the Netherlands may substantially influence the effect of the prison experience on criminal career development. Because sentence lengths in the US are longer and prison conditions more harsh than in the Netherlands, the results suggest to us that the experience of imprisonment in the US may well be even more criminogenic than in the Netherlands. The counter argument to this prediction might be that more lengthy sentences and harsher prison conditions add to the special deterrent effect. The limited evidence available on the term and conditions of incarceration does not support this prediction (Green and Winik 2008; Drago et al. 2008). Nonetheless, the question of the generalizabilty of our findings to US can only be established by more analyzes of US data.[19]

Other notable limit to the generalizability of the findings is that they pertain only to men and to persons without prior experience with prison. It would be valuable to examine effects on women, who are a growing proportion of the prison population and on individuals with a prior record of incarceration. Regarding the latter group it would be interesting from both a scientific and policy perspective to determine whether the criminogenic effect suggested by our analysis is diminished or even reversed in a population who had already experienced imprisonment. We further note that even though we limit our analysis to sentences of less than 1 year there is considerable heterogeneity in the conditions of incarceration (e.g., in an closed, half-open or open prison). It would be desirable to replicate this analysis with more homogenous conditions of confinement.

Nevertheless, if we rely on our Dutch results indicating that the experience of imprisonment is criminogenic, our findings have important implications for criminal justice policy and interventions. The results imply that there is a trade-off between the crime prevention effect of incapacitation and general deterrent and the increase in crime that arises from the criminogenic effect of the experience of imprisonment of those actually imprisoned. A rigorous assessment of this trade-off requires as yet unavailable estimates of the effect of imprisonment on the imprisoned as it varies by the number of prior episodes of imprisonment and the length of incarceration. It also requires the development of analytical models for assessing this trade-off. The greatest challenge to developing this analytical capability is modeling the dynamic dimension of the analysis because the criminogenic effects of incarceration create feedbacks that must be accounted for in predicting future crime rates.

Furthermore, if we believe our findings and conclusions on the criminogenic effects of imprisonment, then these have important implications for both criminological theories and empirical research. From a theoretical perspective the results challenge the theory of specific deterrence. Experiencing imprisonment does not seem to deter future offending and even if prison does make offenders think again, other processes seem to prevent these thoughts from translating into desistance. The analysis does not probe the mechanism by

[19] We also note that the US itself is very large and diverse country. For this reason it is not self evident that findings based on a Hispanic population from the Southwestern US are any more generalizable to rural New England than are results from the Netherlands.

this criminogenic effect arises. The source of the increase might be due to experiences during the period of incarceration. The experience of first-time imprisonment might, however, also have indirect consequences post release. Former inmates might lose their job and possibly their place of residence, which increases chances of reoffending.[20] We recommend that future research on the effect of imprisonment on the imprisoned give priority attention to distinguishing among the potential mechanisms by which an effect might be generated.

The finding that only limited evidence of differences across trajectory groups in the response to first-time imprisonment also has theoretical implications. In the case of violence, there was no statistically significant effect for the chronic group whereas there were significant effects for the other two groups. Tests of whether the effect size was significantly different between the chronics and each of the other two groups showed that it was. However, for other crime types there were no significant differences across trajectory groups. So, the limited evidence of effect differences across trajectory group may imply that theoretical arguments for dependencies based on developmental history do not apply to the experience of imprisonment. However, it is to be noted that the limited evidence may also reflect the fact that our analysis did not include individuals with a high propensity for imprisonment because of a lack of suitable matched controls. These high propensity individuals tended to be disproportionately from the chronic trajectory. Thus, our matched chronics are not representative of the total population of chronics.

We close with the observation that the dearth of evidence on the effects of imprisonment on the future criminality of the imprisoned is startling in light of the fact that incarceration is the predominate sanction for serious crime in the Western World. More research on this topic should be a high priority.

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[20] Note that having been incarcerated may make it more likely that, ceteris paribus, an individual will subsequently be convicted of a later crime, as a result of a labeling process by judges. That is, if the take prior prison time as indicators of bad character, they might suffice with less tangible evidence to reach a guilty verdict.

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